The Labor Market Impact of Refugees:

Evidence from the U.S. Resettlement Program

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The Labor Market Impact of Refugees: Evidence from the U.S. Resettlement Program[1]

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Abstract

In this paper, we examine the long-term impact of refugees on the U.S. labor market over the period 1980-2010. Drawing upon aggregated individual level administrative data, we exploit the exogenous assignment of refugees across commuting zones within the United States, by focusing on those refugee cases *without U.S. ties*. The distribution of these refugees depends upon resettlement agencies and is mainly driven by the availability of accommodation and other (for example medical) needs – most importantly, it is independent of the choice of refugees. Nevertheless, we use matching techniques to identify suitable counterfactual observations for those commuting zones that receive a significant number of refugees as a share of the population. Accounting for all of the recent innovations in the literature, we do not find any significant long-term labor market impact of refugees. Our results provide robust causal evidence that there is no adverse long-run impact of refugees on the U.S. labor market.

[1] This paper is part of a broader research agenda by one of the authors, Anna Maria Mayda, to shed light on the economic impact of refugees resettled to the United States. Anna Maria Mayda thanks the Bureau of Population, Refugee and Migration (PRM) at the U.S. State Department for providing the data on which this analysis is based. Anna Maria Mayda worked on this analysis when she was Senior Economist at the Office of the Chief Economist at the U.S. State Department. The authors also thank Chief Economist Keith Maskus, Guy Lawson, Glenn Sheriff and seminar participants at presentations at PRM for insightful comments. All errors remain ours.

1. Introduction

Since the ratification of the 1980 Refugee Act, the U.S. Refugee Admissions Program (USRAP) has resettled more than three million refugees across the United States, making it the largest resettlement program globally. Despite the importance of the program, its economic impact has received little attention from academics, largely because of a lack of empirical evidence, which in turn is due to the paucity of available data with which to conduct analyses. In this paper we offer new empirical evidence by analyzing one particular aspect of the economic effect of refugee resettlement to the United States: the impact on local labor markets.

The impact of refugees on local U.S. communities can take place through various economic channels. Refugee arrivals in a host community typically represent an increase to the supply of workers, making the labor market one of the most important channels of impact. Other economic mechanisms work through the government budget, changes in the prices of goods and services, and impacts on international trade flows. In this paper we investigate the labor market channel using the Worldwide Refugee Admissions Processing System (WRAPS) data set[2] housed at the Refugee Processing Center (RPC) – which is part of the Bureau of Population, Refugees and Migration (PRM) at the U.S. Department of State. We empirically analyze the impact of refugee arrivals to a U.S. commuting zone on the labor market outcomes of natives, including their wages and employment by skill levels in that locality. Our results provide robust causal evidence that there is no adverse long-run impact of refugees on the U.S. labor market.

Standard labor-economics models predict that the impact of refugees through the labor-market channel should be unevenly distributed across the population, i.e. some native workers may lose while others may gain. To the extent refugees resettled to the United States are unskilled or take on unskilled jobs, they are more likely to compete with unskilled U.S. workers and complement skilled U.S. workers. In other words, their arrival could induce a decrease in the unskilled wage and an increase in the skilled wage (Borjas 1999, 2003). At the same time, the local economy may adjust to the arrival of refugees along other dimensions. Examples include through changes in production patterns (i.e. Rybczynski effects), firms adopting alternative production techniques (Lewis 2011), natives' outflows to other labor markets (Borjas 2006), investment by natives in education (Hunt 2012) and occupational upgrading (Peri and Sparber 2009, Ottaviano, Peri and Wright 2013). In all these cases, the labor-market effects can be attenuated or reversed (Lewis and Peri 2014).

An extensive literature has empirically analyzed the labor-market impact of immigration to the United States (Borjas 2014, Lewis and Peri 2014) as well as to other countries (see for example Dustmann, Frattini and Preston

[2] Refugee records in WRAPS are protected under Section 222(f) of the Immigration and Nationality Act, 8 U.S.C. § 1202(f), and may be subject to the Privacy Act of 1974, as amended, 5 U.S.C. § 552a. Therefore, the WRAPS dataset is not publically available.

2013 and Glitz 2012). The main challenge in this body of literature has been to identify a *causal* effect as opposed to a simple correlation. Endogeneity, and in particular reverse causality, is an issue in the analysis of refugee resettlement as well. Refugees' decisions regarding where to live and work within the United States are a function of the very same variables we aim to analyze as outcomes – such as wages and employment opportunities at the destination (see Borjas 1999). In this paper, we address endogeneity concerns by exploiting information on the *initial* placement within the United States of refugees *without U.S. ties*, who report no family members or friends already in the United States. If a refugee does not report a U.S. tie, resettlement agencies will place his/her case somewhere within their national network. Importantly, agencies are required to place refugees in locations close to their offices. Among possible locations agencies take into consideration factors such as housing needs, availability of medical/specialized/social services, the characteristics of the case and the ability of various local programs and communities to meet those needs, the existence of a diaspora community from the same country of origin, and labor market conditions. This could imply that the placement of refugees with no U.S. ties across CZs, by resettlement agencies, is non-random, in the sense that pre-treatment trends of the outcome variables may be different for treatment CZs (i.e. those which receive refugees) and control CZs (i.e. those without refugees). To address this issue, we follow a similar methodology to Dustmann, Schoenberg and Stuhler (2017) and use matching to select the sample of "control" CZs, – among those which do not receive refugees – so that they are comparable, on the basis of economic characteristics before the period of analysis, to the "treatment" CZs – which are those receiving refugees.

The rest of the paper is organized as follows. In Section 2 we summarize the literature related to this paper. In Section 3 we provide background about the U.S. Refugee Admissions Program, including its history and the way it works in practice, starting with overseas processing and ending with resettlement in the United States. In Section 3 we describe the WRAPS data set. In Section 4 we develop the empirical strategy while, in Section 5, we present the empirical results. Section 6 concludes.

2. Related literature

The literature on the labor-market impact of migration is extensive and has been reviewed in a number of recent papers (see for example Borjas 2014, Lewis and Peri 2014 and Peri 2016). On the other hand, the related literature on the labor-market impact of *refugees* is far smaller.[3] That literature is more closely relevant to this paper, and the focus of this section.

The "natural experiment" approach was pioneered in David Card's (1990) study of the Mariel Boatlift, the arrival of about 125,000 Cuban refugees in 1980, to Miami, the largest location in which they settled. The approach used

[3] For recent survey articles,see Ruiz and Vargas-Silva (2013) and Mabiso et al. (2014).

to analyze the impact of the Mariel boatlift is essentially a difference-in-difference approach. Changes over time in Miami are compared to those in comparable other cities that did not experience the arrival of Cuban refugees. Evidence from Card (1990) and Peri and Yasenov (2015) suggest that the boatlift broadly did not have a significant impact on Miami's labor market. Average wages in Miami were broadly unaffected. However, as Borjas (2016) finds, there were significant wage decreases for the specific native group of non-Hispanic males, high school dropouts.

The end of the Cold War was accompanied by large flows of people across national borders. Among the main studies that focus on this period are several papers that investigated the impact of over 600,000 immigrants from the former Soviet Union moving to Israel.[4] Aydemir and Kirdar (2013) explore the impact of the arrival of ethnic Turks from Bulgaria in 1989. Glitz (2012) analyzes the inflow of 3 million ethnic Germans moving from Eastern Europe and the Soviet Union to Germany. Importantly, from a methodological perspective, Germany instituted a dispersal policy for these immigrants, placing them quasi-randomly across Germany. The results indicate a displacement effect of 3.1 unemployed workers for every 10 immigrants that find a job in a given region in Germany, but no effect on relative wages.[5]

Dustmann, Schönberg and Stuhler (2016) analyze a post-1989 policy that allowed Czech workers to seek employment, but denied residence rights, in eligible German border municipalities. The study first finds appropriate control regions throughout Germany, akin to the synthetic control approach, but then also provides instrumental variable estimates based on a region's distance from the Czech border. After three years of the policy, a 1 percentage point increase in the inflow of Czech workers relative to native employment had led to about a 0.13 percent decrease in native wages, and – an almost one-to-one – 0.93 percent decrease in native local employment.

There have been several studies looking at the return of expatriates from former colonies after these colonies became independent. These include a study of repatriates from Algeria to France (Hunt 1992), and of African repatriates to Portugal (Carrington and Lima 1996). There is work on the impact of the refugee flows from the breakup of Yugoslavia (Angrist and Kugler 2003). Displaced people are the subject of Mansour's (2010) work on the West Bank, and Braun and Mahmoud (2014) study the impact of expelled ethnic Germans after World War II.

[4] These include Friedberg (2001), Lach (2007), and Paserman (2013).

[5] See also Damm (2009), which uses a Danish dispersal policy implemented through the provision of public housing to study the impact of immigration.

Foged and Peri (2013) study refugee flows - dispersed across Denmark by a refugee dispersal policy - on measures of occupational complexity and manual intensity of the occupations engaged in by Danish natives.[6] The estimates show that an increase of the supply of low-skilled refugees pushes less educated native workers (especially the young and low-tenured) to pursue less manual-intensive occupations and those with a higher degree of complexity. The impact is remarkably stable across time, suggesting that these changes are permanent.

The current Syrian refugee crisis is the subject of a number of papers. Del Carpio and Wagner (2016) analyze the impact of recent Syrian refugee inflows on the Turkish labor market. The authors find that, on average, for every 10 refugees, three Turkish workers lose employment. This result hides interesting dynamics across the formal vs. informal labor markets. In the period analyzed (between 2011 and 2014), most Syrian refugees lived outside of refugee camps and were not issued formal work permits in Turkey. The authors find evidence of displacement effects (and wage decreases) of native workers in the informal labor market – where Syrian refugees and Turkish workers were closer substitutes – at the rate of around six native workers losing their informal jobs for every 10 refugees. At the same time, the refugee inflow increased the propensity of Turkish workers to be formally employed, at the rate of around three native workers finding formal employment for every 10 refugees. This is an example of occupational upgrading. However, the effects are different across socio-economic groups: formal employment rises for Turkish men who have not completed a high school education, but not for women and highly-skilled men. The net impact on employment is negative for women and for the least educated Turkish workers—both groups lose informal jobs and exit the labor force.[7]

Finally, it is worth mentioning papers that focus on the impact of refugees in camps. A substantial fraction of the world's refugees are housed in camps: an estimated 21 percent in 2015 (UNHCR 2016). The existence of these camps substantially complicates our understanding of the impact of refugee flows as in these instances the impact extends well beyond a simple labor supply shock. Maystadt and Verwimp (2014) identify how the local population has been affected by the refugee inflows from Burundi in 1993 and Rwanda in 1994 in the region of Kagera in northwestern Tanzania. On average, they find that doubling the presence of refugees increases real consumption (in per adult equivalents) by about 8 percent. These benefits are not equally distributed: those initially working as agricultural workers or self-employed in nonagricultural activities gain 3 to 4 percentage points less than the rest of the population. The authors argue that the relative loss of the agricultural workers can be explained by the fiercer competition encountered in labor markets. The special nature of refugee camps even

[6] The complexity index is increasing in communication and cognitive content while it is decreasing in manual content of occupations.

[7] Ceritoglu et al. (2015) use a difference-in-difference strategy comparing changes to outcomes in Turkish border regions with camps with those for a control group of regions (in eastern Anatolia to obtain qualitatively similar findings. In a similar vein, providing evidence from Colombia Calderón-Mejía and Ibáñez (2015) argue that internally displaced people compete primarily in the informal sector in host communities. The estimates suggest that these migrations substantially reduce wages for urban unskilled workers who compete for jobs with forced migrants in the informal sector.

generates plausible long-term effects on the local economy. Duranton and Maystadt (2014) exploit a 1991–2010 Tanzanian household panel to assess the effects of the temporary refugee inflows originating from Burundi (in 1993) and Rwanda (in 1994). The study finds that the refugee presence has had a persistent and positive impact on the welfare of the local population. The authors argue that the most likely explanation is the reduction in transportation costs arising from the construction of major roads to supply the refugee camps.

3. Background

3.1. History of the U.S. Refugee Admissions Program

In 1945 President Truman passed a directive granting 'Welfare Organizations' the power to sponsor refugees, provided that they covered all associated costs and that the individual in question had a relative in the United States. This preceded the signing of the Displaced Persons Act of 1948, which acknowledged refugees as a special class of migrant for the first time and, together with its extension in 1950, paved the way for hundreds of thousands of displaced Europeans to enter the United States. In subsequent decades, the United States continued admitting refugees across its borders largely from Communist countries. Most of these subsequent waves of refugees were resettled by private ethnic and religious organizations, some of which had been operating in one form or another for decades, and which, to this day, form the institutional backbone of the public-private partnership model of the U.S. Refugee Admissions Program.

The watershed 1965 Hart-Celler Act ended the national origins formula for migrant quotas and finally defined refugees as constituting separate legal entities relative to other immigrants, but *only* for Europeans fleeing communism. This restricted definition led to the passing of the 1966 Cuban Adjustment Act to provide political asylum to Cubans that reached U.S. soil and the Indochina Migration and Refugee Assistance Act of 1975, which granted special status to the first wave of Indochinese refugees who entered the United States after the fall of Saigon (see Figure 1). After 1975, the United States resettled hundreds of thousands of Southeast Asian refugees via an interagency task force established with temporary funding. This reportedly chaotic experience proved the catalyst for Congress to pass the Refugee Act of 1980. At that time the United States adopted the U.N. definition of a refugee, standardized resettlement services for refugees and, finally, established into law the Office of Refugee Resettlement (ORR), an office within the U.S. Department of Health and Human Services. Since 1975, the United States has resettled over 3 million refugees, with annual arrivals ranging from 207,000 in 1980 to just 27,110 in 2002.

3.2. Overseas Processing

Given that the Refugee Act of 1980 does not place a statutory limit on refugee arrivals to the United States, each year, the President, after consultation with Congress, determines an annual cap on refugees from each region of the world, known as the 'refugee ceiling'. In Fiscal Year 2016, 84,994 refugees of the 85,000 target were resettled.

The United States processes refugees at Resettlement Support Centers (RSCs) overseas prior to their arrival to the United States. The first step that most refugees take once outside their country of origin is to register with the United Nations High Commissioner for Refugees (UNHCR), which is mandated to provide protection to refugees. This registration is conducted in whichever country to which that particular individual has fled. The United States funds the UNHCR to review refugee registration documents and conduct refugee status determination (RSD) on behalf of the government of the country of asylum. Once recognized as refugees, the UNHCR designates some of them as 'most vulnerable', meaning they are eligible for resettlement. Less than 1% of all 'most vulnerable' refugees are actually resettled anywhere in the world. The process of resettling a refugee to the United States typically takes at least 18-24 months.

Refugees referred by UNHCR to the United States for resettlement are screened for eligibility to access the United States Refugee Admissions Program by one of nine Resettlement Support Centers (RSCs) around the world, working with State Department Foreign Service Officers deployed as Refugee Coordinators. These RSCs are operated by international and nongovernmental organizations under the auspices of the Bureau of Population, Refugees, and Migration (PRM) of the U.S. Department of State. RSCs initiate security screening by relevant U.S. agencies and schedule applicants for interview by DHS/USCIS officers. After interview, RSCs schedule medical exams, request assurance from a domestic resettlement agency, and schedule the refugee's travel through the International Organization for Migration (IOM). The nine RSCs include: Nairobi, which covers all of sub-Saharan Africa, run by the Church World Service;[8] Austria, run by HIAS;[9] Thailand, which covers East Asia, run by the International Rescue Committee;[10] Jordan, covering the Middle East and North Africa, run by the International Organization for Migration;[11] Russia, covering Eurasia, run by the International Organization for Migration;[12] Nepal, covering South Asia, run by the International Organization for Migration;[13] and Turkey, covering Turkey and the Middle East, run by the International Catholic Migration Commission.[14] There also exists a U.S. Government facility in Havana, Cuba. The Worldwide Refugee Admissions Processing System (WRAPS) is used to track refugees' data. Once a refugee has been conditionally approved by the Department of

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[8] See: http://www.churchworldservice.org/site/PageServer?pagename=action_kenya_main
[9] See: https://www.hias.org.
[10] See: http://www.rescue.org/irc-thailand.
[11] See: http://www.iom.int/jahia/Jahia/jordan.
[12] See: http://moscow.iom.int.
[13] See: http://www.iom.int/jahia/Jahia/nepal.
[14] See: http://www.icmc.net/offices/icmc-turkey.

Homeland Security, the RSC requests an assurance of sponsorship from one of nine national resettlement agencies with cooperative agreements with PRM to resettle refugees in the United States. After assurances are received, the refugee's travel to the United States can be booked.

3.3. Resettlement to the United States

PRM is responsible for the processing of refugees prior to their arrival in the United States, and their initial placement and resettlement. ORR is then responsible for providing the newly settled refugees with a range of services for up to five years after arrival in the United States, including medical and cash assistance, English language training, and a range of employment and social services. The Department of State enters into agreements with resettlement agencies (RAs) for reception and placement services during the first three months of the refugee's arrival in the U.S.. These include: Church World Service, Ethiopian Community Development Council, Episcopal Migration Ministries, HIAS, International Rescue Committee, Lutheran Immigration and Refugee Service, U.S. Committee for Refugees and Immigrants, United States Conference of Catholic Bishops/Migration and Refugee Services, and World Relief. These agencies provide an assurance to resettle a refugee in the United States at the local level and do so, from their arrival by meeting refugees at the airport. In turn, these agencies receive a reimbursement by PRM for a proportion of their costs.

The RAs have over 300 offices nationwide and have numerous private volunteers including religious groups, community organizations and private families, which help newly resettled refugees. Representatives of the RAs meet weekly to review refugee cases sent by the RSC to determine where refugees will be allocated. It is at this meeting that the RAs match the needs of refugees with the local resources available to them. As mentioned above, if an incoming refugee already has relatives in the United States, typically he/she is resettled either with them or close to them. Refugees with no U.S. ties have no choice as to where they are placed since their location is determined by the availability of local resources. Unlike in many other countries, refugees in the United States are eligible to work on arrival, although they are unable to work for the federal government, with the exception of the U.S. armed forces. Resettled refugees must apply for lawful permanent residence after one year of residence, while U.S. citizenship may be acquired after five years of continuous residence.

4. Data

The WRAPS data set provides individual-level information for all refugees resettled to the United States, between 1990 and the present. Variables include year of arrival, the city and state of placement within the United States, refugee socio-economic characteristics – e.g. age, gender, marital status, education and occupation of the refugee – the country of origin and whether or not a refugee reports a U.S. tie. Note that the WRAPS data set covers the *universe* of refugees resettled to the United States. One of the advantages of our analysis therefore is that the main explanatory variable is measured with no sampling error, which is not the case for analyses of economic migration since these are instead often based on survey data or subsamples of Census data. We combine the WRAPS data set with information on the total number of refugees by commuting zone, nationality and year, between 1975 and 1990, from a supplementary administrative data set obtained from the ORR.

5. Empirical strategy

Our empirical analysis exploits the variation in the number of refugees as a share of the local population across U.S. commuting zones (CZs) and over time, between 1980 and 2010. County boundaries are not always adequate confines for a local labor market and often reflect political boundaries rather than an area's local economy. For this reason, we focus our attention on variation across CZs, which are geographic units of analysis that more accurately reflect the local economy and labor market where people live and work. Labor-market variables at a disaggregated level (CZs) are constructed using Census or American Community Survey data. For each CZ, we sum refugee arrivals over 10 years and we divide their total by the population at the beginning of the decade. We then estimate their effect on labor-market outcomes across all commuting zones and over three decades (1980-1990, 1990-2000 and 2000-2010) using a panel specification. The exact empirical specification we use is shown below.

We consider a framework in which labor-market outcomes in commuting zone *i* and time *t* can be affected, in the long-run, by the local supply of refugees (as a share of the population). We allow for other factors impacting labor-market outcomes and we capture them with commuting-zone fixed effects, year fixed effects, commuting zone time trends as well as additional time-varying control variables, such as commuting zone populations and measures of labor demand. The basic relation can be written, in linear form as:

$$y_{it} = \beta \cdot f(refstock_{it}) + \delta_i + \delta_t + t * \delta_i + X_{it}\gamma + \varepsilon_{it} \qquad (1)$$

where $t = 1990, 2000, 2010$. In terms of notation, y_{it} is a labor-market outcome (such as log wages or employment) and $f(refstock_{it})$ is a function of the presence of refugees in a given commuting zone *i* and decade t.

Since we have data on refugee arrivals as opposed to stocks, we estimate the above specification in first differences. We focus on changes in the stock of refugees due to *new* resettlements as proxy for $(refstock_{it} - refstock_{i(t-1)})$. We consider the sum of *new* refugee arrivals over all years between $(t-1)$ and t and we define the indicator (treatment) dummy as equal to one for those CZs and decades in which the value of such change in stock, standardized by the initial population of the CZ, was larger than the mean of 0.1%, namely

$\frac{(refstock_{it} - refstock_{i(t-1)})}{Pop_{i(t-1)}}$>0.1%. This threshold implies that the CZs in the top 22% of the distribution of refugees' shares will be considered as "treated".[15,16] Hence, the estimating equation we bring to the data looks as follows:

$$\Delta y_{it} = \beta \cdot I_{it} + \delta_t + \delta_i + \Delta X_{it}\gamma + \varepsilon_{it} \qquad (1')$$

The dependent variable Δy_{it} captures logarithmic or percent changes of labor market variables for native workers. We also include commuting zones and decade effects which capture, respectively, commuting-zone specific trends and national decade trends. We still need be concerned about the following two threats to identification of a causal effect however: i) individual self-selection of refugees into CZs and ii) any non-random placement of refugees across CZs by resettlement agencies.

5.1. Instrumental variable (IV) strategy

Sorting at the individual level arises since refugees are likely to locate in commuting zones where labor-market opportunities have improved. To address this issue we implement an instrumental variable (IV) estimation strategy. As an IV for the indicator variable that takes the value one should the refugee arrivals as a percentage of the population exceed 0.1% we use a corresponding indicator variables that takes the value 1 should the share of *initial* refugees *with no U.S. ties* exceed 0.1%. The placement upon arrival of cases without U.S. ties is decided by resettlement agencies as opposed to by the refugee. In addition, refugees with no U.S. ties have an incentive to stay in their initial location for some time, since otherwise they would lose assistance from the resettlement agency. To summarize, we address the individual self-selection problem by using variation in the initial placement of refugee cases without U.S. ties within the United States. If our IV is estimated in a reduced form equation we effectively examine the Intention to Treat (ITT); whereas our two-stage estimations rather estimate the Average Treatment Effect of the Treated (ATE).

If a refugee does not report a U.S. tie, resettlement agencies will place his/her case somewhere within their national network taking into consideration housing needs, the characteristics of the case, and the ability of various local programs and communities to meet those needs. The availability of medical/specialized/social services and of a diaspora community from the same country of origin are largely the driving factors of the initial placement decision by resettlement agencies of refugees *without U.S. ties*. These driving factors allay some concerns of endogeneity. The resettlement agencies are also required to place refugees in locations close to their offices,

[15] In other words, we do not exploit variation in the stock of refugees due to movement, within the United States, of refugees arrived in the past – given that this variation is likely endogenous.

[16] For robustness we also test thresholds of 0.05% at the 68th percentile and 0.2% the threshold at the 87th percentile.

however,[17] and within the group of CZs where there are offices, resettlement agencies may carry out "strategic placement", i.e. they may select the ones where the labor market is stronger. This could imply that the placement of refugees with no U.S. ties across CZs, by resettlement agencies, is non-random, in the sense that pre-treatment trends of the outcome variables may be different for treatment CZs (i.e. those which receive refugees) and control CZs (i.e. those without refugees). To address this issue, we adopt a similar methodology to Dustmann, Schoenberg and Stuhler (2017), and use matching techniques to select a sample of "control" commuting zones (CZs) – among those which do not receive refugees – so that they are comparable, on the basis of economic characteristics before the period of analysis, to the "treatment" CZs – which are those receiving refugees.

Note that, for the 1990s and 2000s, we have access to individual-level information on whether a case is with or without U.S. ties and its initial placement at a detailed geographical level – we also know other individual-level characteristics such as the country of origin. Hence it is straightforward to construct the instrument based on variation across CZs in the number of refugees with no U.S. ties. Specifically, for these two decades, we construct the instrument as follows: (i) we calculate the number of arrivals of refugees with no U.S. ties, by CZ and decade; (ii) we divide by the population of the CZ at the beginning of the decade. On the other hand, for the 1980s, we do not have individual level data on each type of refugee case (i.e. with or without U.S. ties). For the 1980s therefore, we construct the instrument as follows: (i) we calculate the distribution of refugees from Vietnam, Laos and Cambodia, across CZs between 1975 and 1979;[18] (ii) we calculate the total number of refugee arrivals from these countries of origin between 1980 and 1989 and multiply these totals by the 1975-1979 distribution in (i) across CZs; (iii) we divide by the population of each CZ in 1980. Finally, we append the instrument for each decade and interact it with decade dummy variables.

5.2. Matching of commuting zones

We adopt a similar identification strategy to Dustmann, Schoenberg and Stuhler (2017) in the sense that we identify suitable control observations through a matching exercise before testing the impact on labor market variables of our exogenous refugee shocks. We define a decade-invariant set of CZs as our sample and run panel regressions on this sample. If the refugee share for a given CZ is above a certain threshold (0.1%) in any decade, then we classify it as a "treated" CZ. For these "treated" CZs, we use characteristics from the 1970 Census to match them to a set of "control" CZs.

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[17] One of the rules for resettlement of refugees is that their placement has to be within a certain number of miles of one of the offices of resettlement agencies.

[18] Note that: 1) there were no nationals from those countries in the United States prior to 1975 (based on IPUMS Census data); 2) we only consider the first five years from the beginning of the wave so that it is more likely that we are capturing no-U.S.-ties cases. If we follow the same two steps using data from the 1990s and 2000s, we select five "initial waves" (from Bhutan, Burundi, Congo, Rwanda and Togo): Using the WRAPS data set, we confirm that refugees from these countries in the first five years of the wave were indeed composed almost entirely of cases with no U.S. ties.

Following the methodology of Imbens (2015) we identify suitable control units for each of our treated observations by conducting a series of Likelihood Ratio tests on incrementally more complex models. This method identifies a “control” unit for each treated observation, which is similar along a set of covariates (matching variables). We test a comprehensive set of matching variables including: employment rates (total, low skilled, foreign born, high school dropout, college, more than college, black), wages (total, foreign born, high school dropout, college, more than college, black, construction, manufacturing), wage inequality (total, foreign born), size of manufacturing and construction sectors and total size of the commuting zone as captured by total population.

The value of 0.1% is chosen as the threshold for two reasons i) it is around the mean of our refugee share variable $\frac{refstock_{it}}{Pop_{i(t-1)}}$ and ii) it ensures a good balance between treating a sufficient number of units on the one hand while leaving enough units to serve as meaningful controls. We use Mahalanobis nearest neighbor matching and follow Abadie and Imbens (2006, 2011) in adjusting for the bias when matching on two or more continuous variables. Adhering to this methodology we achieve a tight match between treated and non-treated observations as should in Table 1, which additionally shows the results of t-tests between the two samples for each variable that ultimately appeared in the matching equation.

6. Main results

Our main results are shown in Tables 2-5. Each Table shows the estimated coefficients in nine columns, one for each outcome variable. The first three columns show the effects on employment, wages and population change including all native individuals. Columns 4-6 show the coefficients on the same outcomes for low skilled natives (those with high school diploma or lower education) and columns 7-9 show the coefficients on the outcomes for highly skilled natives, those with at least some college education. All regressions include commuting-zone and year fixed effects. All are weighted by the native population of the commuting zone to account for the different measurement error of the dependent variable, which is inversely proportional to the size of the cell. We include a set of controls that should capture other local economic conditions potentially affecting both inflows of immigrants and labor market outcomes. These are a Bartik control (from Bartik 1991), based on national employment growth at the industry level and employment share across industries in the CZ as of 1980. This indicator captures the growth of local employment predicted by industrial composition. We also include a similar Bartik based control that captures local wage growth predicted by industrial composition, as well as the initial population level of the CZ and the imputed shift-share of all immigrants as a proxy for the local change in immigrants supply based on pre-existing communities and recent inflows by nationality. Standard errors are clustered at the commuting zone level.

Table 2 shows the results from a panel regression of the outcomes on the "treatment dummy" which is based on the initial settlement of free refugees and equals one when this inflow in the decade as share of the initial population is larger than 0.1. The regression includes all the CZs nationwide and relies on the fact that the initial dispersal of refugees was not based on pre-existing trend of the labor market. Table 3 shows the same regression, but limited to the set of treated and matched-control commuting zones.

The main result shown by the coefficient of the first row is that all the labor market effects of natives are estimated to be very small, in the order of fractions of one percent and the standard error is also relatively small (around 1-2 percent). In terms of average wage and employment effect one can rule out any long-run effect larger that 2-3 percent (positive or negative) and the effects on less educated do not show any evidence of being significantly different from those. While the average refugee inflow was not large and hence one would not expect a sizeable long-run effect, these estimates rule out strong long-run effects in terms of displacement or competition with native workers. It should be noted however, the refugees resettled across the United States are likely very different from natives and so one should exercise caution before generalizing our results.

Notice that the control variables in the regression have the expected sign and some are quite important to predict employment and wage. The Bartik variables, that should capture sector-driven changes in local labor demand, are very significantly correlated with changes in employment and wages of natives. The initial population is negatively related to population changes, which makes sense if population is around a steady state, and the imputed shift-share of immigrants is also sometimes correlated with employment and population changes. We also tested whether the imputed shift-share, that predicts overall immigration, is correlated to the refugee treatment and we found no significant correlation. This boosts our confidence in the resettlement process as determining refugee location in a way that is independent from what they would have chosen themselves, driven by economic and network considerations.

A final observation is in place when looking at Table 2 and 3. While matching certainly improves the similarity between treatment and control and the more balanced statistics in the matched sample between treatment and control are an important check, the estimation results are not very different between the full sample and the matched sample estimates. This means that, while the group of treated commuting zones is not a purely random-subsample of all U.S. counties, as some covariates are not equally distributed between the two, the dispersal policy distributed refugees across commuting zones in a way that does not seem to correlate with the relevant labor market variables.

Table 4 and 5 show the same panel regressions of outcomes on the treatment dummy and control, when the dummy is defined using actual refugees and instrumented with the initial free-distributed ones (the IV directly used in Table 2 and 3). The estimates of 2SLS are similar in magnitude and significance to those of the reduced form regressions.

Table 6-7 and 8-9 show the reduced form results when we define the "treatment" shock to be broader (all CZ receiving more than 0.05% of the population in refugees) or more concentrated (inflow larger than 0.2% of population). Even in this case the results remain largely small and insignificant, with only a marginally significant negative impact on native population for the larger shocks (Table 8). Potentially, the largest inflows of refugees may have been accompanied by some small population change. The corresponding regression on the matched sample, however, does not show any significant effect implying that it may be the case that the distribution of refugees across CZs may have been selected into those that were experiencing native population decline and, once this is accounted for, refugees did not have any causal impact on this phenomenon.

Specifications 10-15, finally, omit some controls (either one Bartik, in Table 9-10, or the initial population in Tables 11-12, or the immigrant shift-share in Table 13-15). None of these omissions produces measurable changes on the estimates of the effect of the treatment dummy, again implying that the dispersal of refugees was not very correlated with those features of the local economies.

Overall, the initial resettlement process of refugees, in the aftermath of large humanitarian crises across the world, which distributed several million migrants across U.S. commuting zones over the period 1980-2000, has provided us with an exogenous source of variation in the distribution of refugees. The resettlement of these refugee arrivals was not strongly correlated with the location of other immigrants. Our regressions show that commuting zones that received a large inflow of refugees did not experience different labor market outcomes relative to similar commuting zones that did not receive refugees. While the inflow of refugees was small as a percentage of the population, our results estimate effects smaller than a fraction of a percent on native wages, and the standard error rules out wage effects outside the + or – 2 percent range for both native skilled and unskilled.

7. Conclusions

In this paper, we analyze the impact of refugees resettled to the United States on the labor-market outcomes of native workers between 1990 and 2010. The empirical analysis exploits (exogenous) variation in refugee cases "without U.S. ties" – who do not decide the initial location of resettlement within the U.S. – and compares outcomes of "treatment" vs. "control" commuting zones where the latter are selected based on a similar approach to Dustmann, Schoenberg and Stuhler (2017) in that we use an IV approach on matched units. Our results provide robust causal evidence that there is no adverse long-term impact of refugees on the U.S. labor market.

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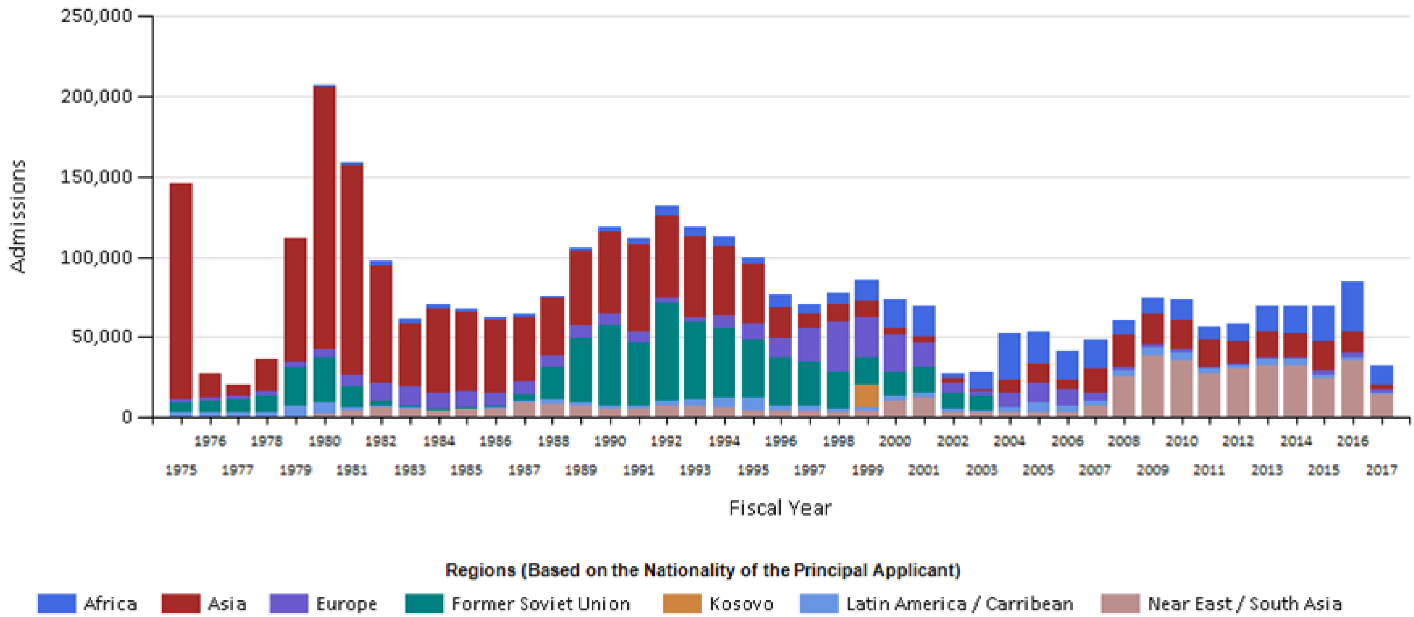
Table 1: Balance Statistics of Matching Exercise: Threshold 0.1%

	Treated	Control	%bias	t	p>t
Employment Rate	0.77562	0.77537	0.3	0.05	0.964
Employment Share Low Skilled	0.45506	0.45472	0.4	0.05	0.959
Employment Share Foreign Born	0.04843	0.04897	-0.9	-0.14	0.892
Total Weekly Wages	6.118	6.1168	0.4	0.06	0.954
Weekly Wages Foreign Born	6.0222	6.0199	0.8	0.09	0.925
Employment Share High School Dropout	0.12383	0.12388	-0.1	-0.01	0.993
Employment Share More than College	0.06128	0.06158	-1.1	-0.21	0.833
Wage More than College	6.6777	6.6773	0.1	0.02	0.986
Employment Share College	0.14383	0.14372	0.3	0.04	0.969
Employment Share Black	0.01901	0.01833	0.7	0.2	0.841

Figure 1: Refugee Admissions to the United States by Origin, 1975-present day



Source: WRAPS databas

Table 2: Full Sample: Reduced Form Regressions

	(1) Change Total Log Wage	(2) Change Total Log Employment	(3) Change Total Population %	(4) Change LS Log Wage	(5) Change LS Log Employment	(6) Change LS Population %	(7) Change HS Log Wage	(8) Change HS Log Employment	(9) Change HS Population %
Refugee Treatment	-6.76E-05	-0.000364	-0.00214	0.00138	0.00269	-0.0113	-0.00251	0.00565	0.00169
	[0.00850]	[0.0103]	[0.0109]	[0.00853]	[0.0118]	[0.0124]	[0.00803]	[0.0114]	[0.0223]
Bartik Wage	1.004***	0.387***	0.180***	1.072***	0.270***	0.0598	0.891***	0.415***	0.254*
	[0.0434]	[0.0566]	[0.0661]	[0.0461]	[0.0698]	[0.0625]	[0.0492]	[0.0871]	[0.145]
Bartik Employment	0.0284	0.482***	0.288***	0.0841***	0.584***	0.249***	0.00062	0.433***	0.392***
	[0.0250]	[0.0389]	[0.0327]	[0.0312]	[0.0423]	[0.0284]	[0.0277]	[0.0449]	[0.0627]
Ln(Population)	-0.0599***	-0.155***	-0.182***	-0.031	-0.0356	-0.0138	-0.0352	-0.101**	-0.241***
	[0.0215]	[0.0348]	[0.0350]	[0.0203]	[0.0314]	[0.0425]	[0.0215]	[0.0391]	[0.0686]
Immigrant Shift Share	0.0581	-0.622**	-0.635**	-0.188***	0.0634	-0.514	0.216	-0.469**	-0.802**

	[0.120]	[0.241]	[0.322]	[0.0677]	[0.115]	[0.369]	[0.139]	[0.185]	[0.327]
Constant	1.304***	2.154***	2.442***	0.869***	0.36	0.071	0.947***	1.774***	3.616***
	[0.280]	[0.455]	[0.459]	[0.265]	[0.412]	[0.555]	[0.281]	[0.512]	[0.899]
Observations	2,166	2,166	2,166	2,166	2,166	2,166	2,166	2,166	2,166
R-squared	0.974	0.925	0.892	0.964	0.901	0.858	0.965	0.933	0.865
Commuting Zone FE	YES	YES	YES	YES	YES	YES	YES	YES	YES
Year FE	YES	YES	YES	YES	YES	YES	YES	YES	YES
Population Weighted	YES	YES	YES	YES	YES	YES	YES	YES	YES

All standard errors clustered at the commuting zone level.

*** p<0.01, ** p<0.05, * p<0.1

Table 3: Matched Sample: Reduced Form Regressions

	Change Total Log Wage	Change Total Log Employment	Change Total Population %	Change LS Log Wage	Change LS Log Employment	Change LS Population %	Change HS Log Wage	Change HS Log Employment	Change HS Population %
Refugee Treatment	-0.00998	-0.00562	-0.00949	-0.00261	-0.000493	-0.0168	-0.0209	-0.0326	-0.0295
	[0.0146]	[0.0214]	[0.0258]	[0.0148]	[0.0214]	[0.0139]	[0.0156]	[0.0506]	[0.0633]
Bartik Wage	1.010***	0.461***	0.187***	1.090***	0.129*	0.0618	0.870***	0.348***	0.167
	[0.0650]	[0.0806]	[0.0550]	[0.0714]	[0.0722]	[0.0676]	[0.0826]	[0.129]	[0.175]
Bartik Employment	-0.0255	0.519***	0.236***	0.0198	0.704***	0.189***	-0.0268	0.459***	0.414***
	[0.0798]	[0.0956]	[0.0626]	[0.0949]	[0.0980]	[0.0448]	[0.0802]	[0.0769]	[0.130]
Population	-0.0712*	-0.118	-0.171**	-0.0457	-0.0481	-0.137***	-0.0473	-0.0796	-0.247
	[0.0366]	[0.0832]	[0.0726]	[0.0464]	[0.0506]	[0.0491]	[0.0464]	[0.112]	[0.202]
Shift Share	-0.098	-0.347***	-0.114	-0.231***	0.00748	-0.0349	0.0606	-0.299*	-0.192
	[0.0674]	[0.0891]	[0.105]	[0.0608]	[0.0968]	[0.134]	[0.0754]	[0.169]	[0.163]
Constant	1.451***	1.667	2.291**	1.061*	0.544	1.698***	1.108*	1.489	3.694

	[0.482]	[1.091]	[0.956]	[0.610]	[0.665]	[0.642]	[0.610]	[1.474]	[2.654]
Observations	1,093	1,093	1,093	1,093	1,093	1,093	1,093	1,093	1,093
R-squared	0.971	0.904	0.882	0.957	0.888	0.89	0.96	0.909	0.842
Commuting Zone FE	YES	YES	YES	YES	YES	YES	YES	YES	YES
Year FE	YES	YES	YES	YES	YES	YES	YES	YES	YES
Population Weighted	YES	YES	YES	YES	YES	YES	YES	YES	YES

All standard errors clustered at the Commuting zone level.

*** p<0.01, ** p<0.05, * p<0.1

Table 4: Full Sample: 2-stage Instrumental Variable Regressions.

	Change Total Log Wage	Change Total Log Employment	Change Total Population %	Change LS Log Wage	Change LS Log Employment	Change LS Population %	Change HS Log Wage	Change HS Log Employment	Change HS Population %
Refugee Treatment	0.0111	-0.0277	-0.0335	0.00698	-0.0228	-0.0474	-0.0122	0.0178	-0.0365
	[0.0515]	[0.0765]	[0.0831]	[0.0447]	[0.0636]	[0.0994]	[0.0479]	[0.0740]	[0.141]
Bartik Wage	1.011***	0.369***	0.159*	1.076***	0.254***	0.0335	0.884***	0.424***	0.23
	[0.0593]	[0.0769]	[0.0886]	[0.0534]	[0.0771]	[0.103]	[0.0563]	[0.0798]	[0.162]
Bartik Employment	0.0277	0.484***	0.290***	0.0836***	0.585***	0.253***	0.00149	0.432***	0.394***
	[0.0200]	[0.0323]	[0.0284]	[0.0245]	[0.0342]	[0.0260]	[0.0222]	[0.0361]	[0.0512]
Population	-0.0602***	-0.154***	-0.181***	-0.0313*	-0.0353	-0.0118	-0.0347*	-0.102***	-0.240***
	[0.0188]	[0.0291]	[0.0297]	[0.0175]	[0.0254]	[0.0365]	[0.0184]	[0.0318]	[0.0573]
Shift Share	0.0711	-0.653**	-0.672**	-0.180***	0.0358	-0.563	0.203	-0.451**	-0.845**
	[0.129]	[0.262]	[0.332]	[0.0651]	[0.120]	[0.381]	[0.136]	[0.201]	[0.387]
Constant	1.307***	2.146***	2.430***	0.872***	0.358	0.0431	0.940***	1.786***	3.609***

	[0.244]	[0.379]	[0.388]	[0.228]	[0.333]	[0.476]	[0.240]	[0.416]	[0.749]
Observations	2,166	2,166	2,166	2,166	2,166	2,166	2,166	2,166	2,166
R-squared	0.974	0.923	0.888	0.964	0.901	0.851	0.965	0.932	0.863
Commuting Zone FE	YES	YES	YES	YES	YES	YES	YES	YES	YES
Year FE	YES	YES	YES	YES	YES	YES	YES	YES	YES
Population Weighted	YES	YES	YES	YES	YES	YES	YES	YES	YES

All standard errors clustered at the Commuting zone level.

*** p<0.01, ** p<0.05, * p<0.1

Table 5: Matched Sample: 2-stage Instrumental Variable Regressions.

	Change Total Log Wage	Change Total Log Employment	Change Total Population %	Change LS Log Wage	Change LS Log Employment	Change LS Population %	Change HS Log Wage	Change HS Log Employment	Change HS Population %
Refugee Treatment	-0.0386	-0.0199	-0.0286	-0.00778	0.0029	-0.0648	-0.086	-0.131	-0.108
	[0.0467]	[0.0708]	[0.0819]	[0.0504]	[0.0728]	[0.0441]	[0.0616]	[0.154]	[0.196]
Bartik Wage	1.011***	0.461***	0.189***	1.090***	0.129**	0.0637	0.872***	0.351***	0.171
	[0.0507]	[0.0638]	[0.0435]	[0.0559]	[0.0565]	[0.0528]	[0.0649]	[0.108]	[0.142]
Bartik Employment	-0.0257	0.518***	0.236***	0.0197	0.704***	0.189***	-0.0273	0.458***	0.413***
	[0.0612]	[0.0756]	[0.0506]	[0.0742]	[0.0769]	[0.0383]	[0.0601]	[0.0656]	[0.108]
Population	-0.0599*	-0.112*	-0.163***	-0.0434	-0.0489	-0.118***	-0.0224	-0.0414	-0.215
	[0.0323]	[0.0626]	[0.0513]	[0.0316]	[0.0362]	[0.0444]	[0.0470]	[0.0844]	[0.142]
Shift Share	-0.1000*	-0.348***	-0.115	-0.231***	0.00809	-0.0381	0.0558	-0.306**	-0.196
	[0.0535]	[0.0705]	[0.0850]	[0.0489]	[0.0772]	[0.113]	[0.0632]	[0.120]	[0.123]
Constant	1.304***	1.591*	2.180***	1.031**	0.554	1.451**	0.782	0.99	3.280*

	[0.424]	[0.821]	[0.674]	[0.416]	[0.474]	[0.580]	[0.617]	[1.104]	[1.867]
Observations	1,093	1,093	1,093	1,093	1,093	1,093	1,093	1,093	1,093
R-squared	0.97	0.902	0.879	0.957	0.888	0.881	0.953	0.892	0.83
Commuting Zone FE	YES	YES	YES	YES	YES	YES	YES	YES	YES
Year FE	YES	YES	YES	YES	YES	YES	YES	YES	YES
Population Weighted	YES	YES	YES	YES	YES	YES	YES	YES	YES

All standard errors clustered at the Commuting zone level.

*** p<0.01, ** p<0.05, * p<0.1

Table 6: Robustness on Full Sample Reduced Form Regressions: 0.05% threshold

	Change Total Log Wage	Change Total Log Employment	Change Total Population %	Change LS Log Wage	Change LS Log Employment	Change LS Population %	Change HS Log Wage	Change HS Log Employment	Change HS Population %
Refugee Treatment	0.00807	-0.0178	-0.0179	0.00717	0.00827	-0.00803	0.00945	-0.00531	-0.0241
	[0.00866]	[0.0132]	[0.0124]	[0.00931]	[0.0119]	[0.0143]	[0.00785]	[0.0110]	[0.0187]
Bartik Wage	1.013***	0.368***	0.162**	1.079***	0.278***	0.0562	0.902***	0.406***	0.228
	[0.0496]	[0.0622]	[0.0678]	[0.0521]	[0.0693]	[0.0721]	[0.0554]	[0.0954]	[0.154]
Bartik Employment	0.0297	0.479***	0.285***	0.0851***	0.585***	0.248***	0.00211	0.433***	0.388***
	[0.0240]	[0.0381]	[0.0312]	[0.0301]	[0.0419]	[0.0284]	[0.0268]	[0.0458]	[0.0613]
Population	-0.0627***	-0.148***	-0.176***	-0.0336*	-0.0387	-0.0102	-0.0383*	-0.0995**	-0.232***
	[0.0225]	[0.0367]	[0.0372]	[0.0202]	[0.0299]	[0.0452]	[0.0223]	[0.0392]	[0.0710]
Shift Share	0.0349	-0.570***	-0.582**	-0.209**	0.0384	-0.485	0.19	-0.457***	-0.733***
	[0.115]	[0.189]	[0.265]	[0.0865]	[0.132]	[0.326]	[0.133]	[0.168]	[0.267]
Constant	1.332***	2.091***	2.376***	0.896***	0.393	0.0273	0.977***	1.763***	3.534***

	[0.294]	[0.478]	[0.482]	[0.265]	[0.393]	[0.586]	[0.291]	[0.512]	[0.925]
Observations	2,166	2,166	2,166	2,166	2,166	2,166	2,166	2,166	2,166
R-squared	0.974	0.926	0.895	0.964	0.901	0.857	0.965	0.933	0.866
Commuting Zone FE	YES	YES	YES	YES	YES	YES	YES	YES	YES
Year FE	YES	YES	YES	YES	YES	YES	YES	YES	YES
Population Weighted	YES	YES	YES	YES	YES	YES	YES	YES	YES

All standard errors clustered at the commuting zone level.

*** p<0.01, ** p<0.05, * p<0.1

Table 7: Robustness on Matched Sample Reduced Form Regressions: 0.05% threshold

	Change Total Log Wage	Change Total Log Employment	Change Total Population %	Change LS Log Wage	Change LS Log Employment	Change LS Population %	Change HS Log Wage	Change HS Log Employment	Change HS Population %
Refugee Treatment	0.00457	0.0123	-0.00305	0.00487	-0.00143	-0.0189	0.00708	0.0244	0.0176
	[0.00922]	[0.0129]	[0.0126]	[0.0103]	[0.0156]	[0.0123]	[0.0122]	[0.0226]	[0.0275]
Bartik Wage	0.715***	0.223***	0.0936	0.765***	0.255**	0.0639	0.628***	0.301***	0.217
	[0.0756]	[0.0834]	[0.0942]	[0.0935]	[0.115]	[0.0915]	[0.0820]	[0.108]	[0.152]
Bartik Employment	0.0998***	0.462***	0.195***	0.146***	0.577***	0.188***	0.0856**	0.414***	0.328***
	[0.0361]	[0.0478]	[0.0459]	[0.0443]	[0.0642]	[0.0553]	[0.0420]	[0.0691]	[0.0853]
Population	-0.0912**	-0.161***	-0.173**	-0.103**	-0.143**	-0.149***	-0.0594	-0.0742	-0.190*
	[0.0374]	[0.0552]	[0.0674]	[0.0446]	[0.0682]	[0.0548]	[0.0410]	[0.0742]	[0.113]
Shift Share	-0.194	-0.147	0.00667	-0.285	0.323	0.116	0.0118	-0.0111	-0.0114
	[0.238]	[0.225]	[0.240]	[0.208]	[0.301]	[0.278]	[0.327]	[0.320]	[0.411]
Constant	1.706***	2.223***	2.313***	1.815***	1.823**	1.886***	1.242**	1.365	2.879*
	[0.491]	[0.730]	[0.888]	[0.584]	[0.898]	[0.719]	[0.539]	[0.979]	[1.492]

Observations	973	973	973	973	973	973	973	973	973
R-squared	0.951	0.916	0.888	0.93	0.885	0.848	0.929	0.894	0.837
Commuting Zone FE	YES	YES	YES	YES	YES	YES	YES	YES	YES
Year FE	YES	YES	YES	YES	YES	YES	YES	YES	YES
Population Weighted	YES	YES	YES	YES	YES	YES	YES	YES	YES

All standard errors clustered at the commuting zone level.

*** p<0.01, ** p<0.05, * p<0.1

Table 8: Robustness on Full Sample Reduced Form Regressions: 0.2% threshold

	Change Total Log Wage	Change Total Log Employment	Change Total Population %	Change LS Log Wage	Change LS Log Employment	Change LS Population %	Change HS Log Wage	Change HS Log Employment	Change HS Population %
Refugee Treatment	0.00316	-0.0091	-0.0107	0.00298	-0.0182*	-0.0237*	4.30E-05	-0.0109	-0.0271
	[0.00798]	[0.0103]	[0.0110]	[0.00750]	[0.0108]	[0.0121]	[0.00761]	[0.00955]	[0.0207]
Bartik Wage	1.006***	0.383***	0.176***	1.073***	0.260***	0.053	0.892***	0.407***	0.24
	[0.0438]	[0.0569]	[0.0659]	[0.0463]	[0.0718]	[0.0623]	[0.0500]	[0.0927]	[0.149]
Bartik Employment	0.0292	0.480***	0.286***	0.0847***	0.580***	0.244***	0.00068	0.431***	0.385***
	[0.0259]	[0.0392]	[0.0330]	[0.0322]	[0.0438]	[0.0276]	[0.0286]	[0.0465]	[0.0655]
Population	-0.0573***	-0.162***	-0.191***	-0.0287	-0.0508	-0.0324	-0.0350*	-0.110***	-0.263***
	[0.0182]	[0.0330]	[0.0333]	[0.0179]	[0.0331]	[0.0398]	[0.0194]	[0.0381]	[0.0637]
Shift Share	0.0638	-0.638**	-0.653*	-0.183***	0.0297	-0.551	0.217	-0.492**	-0.851**
	[0.126]	[0.256]	[0.337]	[0.0672]	[0.118]	[0.389]	[0.144]	[0.197]	[0.362]
Constant	1.269***	2.253***	2.556***	0.838***	0.564	0.316	0.943***	1.902***	3.917***

	[0.238]	[0.432]	[0.437]	[0.233]	[0.434]	[0.522]	[0.254]	[0.498]	[0.836]
Observations	2,166	2,166	2,166	2,166	2,166	2,166	2,166	2,166	2,166
R-squared	0.974	0.925	0.893	0.964	0.902	0.861	0.965	0.933	0.867
Commuting Zone FE	YES	YES	YES	YES	YES	YES	YES	YES	YES
Year FE	YES	YES	YES	YES	YES	YES	YES	YES	YES
Population Weighted	YES	YES	YES	YES	YES	YES	YES	YES	YES

All standard errors clustered at the Commuting zone level.

*** p<0.01, ** p<0.05, * p<0.1

Table 9: Robustness on Matched Sample Reduced Form Regressions: 0.2% threshold

	Change Total Log Wage	Change Total Log Employment	Change Total Population %	Change LS Log Wage	Change LS Log Employment	Change LS Population %	Change HS Log Wage	Change HS Log Employment	Change HS Population %
Refugee Treatment	-0.0137	-0.00241	0.000868	-0.00984	0.0181	0.00949	-0.0176	-0.0139	-0.00884
	[0.0108]	[0.0108]	[0.0119]	[0.00963]	[0.0121]	[0.0116]	[0.0123]	[0.0158]	[0.0200]
Bartik Wage	0.934***	0.437***	0.221***	1.024***	0.145*	0.0790*	0.803***	0.390***	0.253
	[0.0656]	[0.0541]	[0.0579]	[0.0704]	[0.0839]	[0.0479]	[0.0688]	[0.111]	[0.174]
Bartik Employment	0.019	0.464***	0.226***	0.0644	0.634***	0.205***	-0.00045	0.412***	0.363***
	[0.0524]	[0.0609]	[0.0427]	[0.0611]	[0.0632]	[0.0304]	[0.0529]	[0.0587]	[0.0953]
Population	-0.0639**	-0.145***	-0.186***	-0.0309	-0.0905**	-0.108***	-0.0523*	-0.0972	-0.258**
	[0.0260]	[0.0522]	[0.0531]	[0.0291]	[0.0393]	[0.0402]	[0.0315]	[0.0657]	[0.116]
Shift Share	-0.112	-0.451***	-0.294*	-0.218***	0.0585	-0.133	0.0181	-0.371***	-0.415*
	[0.0749]	[0.0785]	[0.175]	[0.0542]	[0.144]	[0.167]	[0.0907]	[0.131]	[0.219]
Constant	1.357***	2.029***	2.485***	0.868**	1.099**	1.308**	1.174***	1.725**	3.851**

	[0.342]	[0.685]	[0.699]	[0.382]	[0.516]	[0.528]	[0.414]	[0.861]	[1.522]
Observations	1,762	1,762	1,762	1,762	1,762	1,762	1,762	1,762	1,762
R-squared	0.968	0.915	0.888	0.954	0.888	0.874	0.958	0.915	0.844
Commuting Zone FE	YES	YES	YES	YES	YES	YES	YES	YES	YES
Year FE	YES	YES	YES	YES	YES	YES	YES	YES	YES
Population Weighted	YES	YES	YES	YES	YES	YES	YES	YES	YES

All standard errors clustered at the Commuting zone level.

*** p<0.01, ** p<0.05, * p<0.1

Table 10: Robustness on Full Sample Reduced Form Regressions: Single Bartik Control

	Change Total Log Wage	Change Total Log Employment	Change Total Population %	Change LS Log Wage	Change LS Log Employment	Change LS Population %	Change HS Log Wage	Change HS Log Employment	Change HS Population %
Predicted Refugees	-9.85E-05	-0.00339	-0.00355	0.00129	0.000579	-0.0118	-0.00252	0.00241	-0.000293
	[0.00842]	[0.00972]	[0.0103]	[0.00825]	[0.0115]	[0.0120]	[0.00801]	[0.0110]	[0.0211]
Bartik Wage	1.029***			1.147***			0.891***		
	[0.0393]			[0.0434]			[0.0411]		
Bartik Employment		0.590***	0.339***		0.659***	0.266***		0.550***	0.463***
		[0.0358]	[0.0268]		[0.0411]	[0.0256]		[0.0366]	[0.0483]
Population	-0.0682***	-0.161***	-0.185***	-0.0556**	-0.0401	-0.0148	-0.0354	-0.108***	-0.245***
	[0.0232]	[0.0357]	[0.0349]	[0.0238]	[0.0309]	[0.0425]	[0.0232]	[0.0399]	[0.0679]
Shift Share	0.042	-0.714**	-0.678**	-0.235***	-0.000954	-0.528	0.215	-0.568***	-0.863**
	[0.116]	[0.277]	[0.332]	[0.0731]	[0.134]	[0.367]	[0.136]	[0.205]	[0.335]
Constant	1.412***	2.253***	2.488***	1.186***	0.429	0.0863	0.949***	1.880***	3.681***
	[0.302]	[0.467]	[0.458]	[0.310]	[0.406]	[0.556]	[0.303]	[0.521]	[0.891]

Observations	2,166	2,166	2,166	2,166	2,166	2,166	2,166	2,166	2,166
R-squared	0.974	0.913	0.889	0.963	0.896	0.857	0.965	0.926	0.862
Commuting Zone FE	YES	YES	YES	YES	YES	YES	YES	YES	YES
Year FE	YES	YES	YES	YES	YES	YES	YES	YES	YES
Population Weighted	YES	YES	YES	YES	YES	YES	YES	YES	YES

All standard errors clustered at the Commuting zone level.

*** p<0.01, ** p<0.05, * p<0.1

Table 11: Robustness on Matched Sample Reduced Form Regressions: Single Bartik Control

	Change Total Log Wage	Change Total Log Employment	Change Total Population %	Change LS Log Wage	Change LS Log Employment	Change LS Population %	Change HS Log Wage	Change HS Log Employment	Change HS Population %
Predicted Refugees	-0.00987	-0.0431*	-0.0247	-0.00269	-0.011	-0.0218*	-0.0208	-0.0609	-0.0431
	[0.0140]	[0.0223]	[0.0252]	[0.0146]	[0.0204]	[0.0130]	[0.0152]	[0.0518]	[0.0639]
Bartik Wage	0.990***			1.105***			0.849***		
	[0.0350]			[0.0535]			[0.0551]		
Bartik Employment		0.653***	0.291***		0.742***	0.207***		0.561***	0.463***
		[0.0995]	[0.0676]		[0.0911]	[0.0514]		[0.0642]	[0.103]
Population	-0.0628**	-0.129	-0.175**	-0.0521	-0.0512	-0.139***	-0.0385	-0.0878	-0.251
	[0.0304]	[0.0887]	[0.0738]	[0.0365]	[0.0514]	[0.0490]	[0.0401]	[0.116]	[0.203]
Shift Share	-0.0945	-0.586**	-0.211	-0.233***	-0.0592	-0.067	0.0644	-0.480***	-0.278
	[0.0622]	[0.256]	[0.174]	[0.0553]	[0.0966]	[0.162]	[0.0693]	[0.121]	[0.187]
Constant	1.343***	1.836	2.360**	1.144**	0.591	1.720***	0.995*	1.616	3.755
	[0.399]	[1.161]	[0.970]	[0.480]	[0.676]	[0.639]	[0.528]	[1.527]	[2.669]

Observations	1,093	1,093	1,093	1,093	1,093	1,093	1,093	1,093	1,093
R-squared	0.971	0.881	0.876	0.957	0.887	0.889	0.96	0.902	0.84
Commuting Zone FE	YES	YES	YES	YES	YES	YES	YES	YES	YES
Year FE	YES	YES	YES	YES	YES	YES	YES	YES	YES
Population Weighted	YES	YES	YES	YES	YES	YES	YES	YES	YES

All standard errors clustered at the Commuting zone level.

*** p<0.01, ** p<0.05, * p<0.1

Table 12: Robustness on Full Sample Reduced Form Regressions: No Shift Share Control

	Change Total Log Wage	Change Total Log Employment	Change Total Population %	Change LS Log Wage	Change LS Log Employment	Change LS Population %	Change HS Log Wage	Change HS Log Employment	Change HS Population %
Predicted Refugees	-0.000149	0.000507	-0.00125	0.00164	0.0026	-0.0106	-0.00282	0.00631	0.00282
	[0.00835]	[0.00912]	[0.00961]	[0.00884]	[0.0118]	[0.0114]	[0.00768]	[0.0107]	[0.0208]
Bartik Wage	1.001***	0.413***	0.207***	1.080***	0.267***	0.0813	0.882***	0.434***	0.288**
	[0.0414]	[0.0470]	[0.0525]	[0.0495]	[0.0689]	[0.0522]	[0.0447]	[0.0823]	[0.128]
Bartik Employment	0.0266	0.501***	0.308***	0.0900***	0.582***	0.266***	-0.00619	0.448***	0.417***
	[0.0239]	[0.0420]	[0.0387]	[0.0317]	[0.0415]	[0.0355]	[0.0261]	[0.0459]	[0.0623]
Population	-0.0626***	-0.126***	-0.153***	-0.0224	-0.0385	0.00968	-0.0451*	-0.0794*	-0.204***
	[0.0227]	[0.0413]	[0.0439]	[0.0196]	[0.0304]	[0.0517]	[0.0232]	[0.0424]	[0.0771]
Constant	1.339***	1.781***	2.061***	0.756***	0.398	-0.237	1.076***	1.492***	3.135***
	[0.297]	[0.540]	[0.575]	[0.256]	[0.399]	[0.676]	[0.303]	[0.554]	[1.011]
Observations	2,166	2,166	2,166	2,166	2,166	2,166	2,166	2,166	2,166
R-squared	0.974	0.919	0.884	0.964	0.901	0.852	0.964	0.931	0.86

Commuting Zone FE	YES	YES	YES	YES	YES	YES	YES	YES	YES
Year FE	YES	YES	YES	YES	YES	YES	YES	YES	YES
Population Weighted	YES	YES	YES	YES	YES	YES	YES	YES	YES

All standard errors clustered at the Commuting zone level.

*** p<0.01, ** p<0.05, * p<0.1

Table 13: Robustness on Matched Sample Reduced Form Regressions: No Shift Share Control

	Change Total Log Wage	Change Total Log Employment	Change Total Population %	Change LS Log Wage	Change LS Log Employment	Change LS Population %	Change HS Log Wage	Change HS Log Employment	Change HS Population %
Predicted Refugees	-0.00919	-0.00281	-0.00857	-0.00075	-0.000554	-0.0165	-0.0214	-0.0302	-0.0279
	[0.0141]	[0.0202]	[0.0251]	[0.0146]	[0.0212]	[0.0137]	[0.0155]	[0.0491]	[0.0621]
Bartik Wage	1.019***	0.490***	0.197***	1.109***	0.128*	0.0648	0.865***	0.373***	0.184
	[0.0683]	[0.0760]	[0.0515]	[0.0766]	[0.0728]	[0.0675]	[0.0839]	[0.127]	[0.175]
Bartik Employment	-0.0247	0.521***	0.237***	0.0217	0.704***	0.189***	-0.0273	0.462***	0.416***
	[0.0797]	[0.0942]	[0.0624]	[0.0954]	[0.0977]	[0.0446]	[0.0798]	[0.0769]	[0.130]
Population	-0.0640*	-0.0924	-0.163**	-0.0287	-0.0487	-0.135***	-0.0517	-0.0575	-0.233
	[0.0339]	[0.0885]	[0.0709]	[0.0498]	[0.0484]	[0.0448]	[0.0442]	[0.124]	[0.201]
Constant	1.356***	1.331	2.181**	0.838	0.551	1.664***	1.166**	1.199	3.509
	[0.446]	[1.161]	[0.933]	[0.655]	[0.636]	[0.585]	[0.581]	[1.622]	[2.642]
Observations	1,093	1,093	1,093	1,093	1,093	1,093	1,093	1,093	1,093
R-squared	0.971	0.902	0.882	0.957	0.888	0.89	0.96	0.908	0.841

Commuting Zone FE	YES	YES	YES	YES	YES	YES	YES	YES	YES
Year FE	YES	YES	YES	YES	YES	YES	YES	YES	YES
Population Weighted	YES	YES	YES	YES	YES	YES	YES	YES	YES

All standard errors clustered at the commuting zone level.

*** p<0.01, ** p<0.05, * p<0.1

Table 14: Robustness on Full Sample Reduced Form Regressions: No Population Control

	Change Total Log Wage	Change Total Log Employment	Change Total Population %	Change LS Log Wage	Change LS Log Employment	Change LS Population %	Change HS Log Wage	Change HS Log Employment	Change HS Population %
Predicted Refugees	0.000288	0.000552	-0.00106	0.00156	0.0029	-0.0113	-0.00231	0.00625	0.00312
	[0.00888]	[0.00991]	[0.0106]	[0.00867]	[0.0120]	[0.0124]	[0.00825]	[0.0111]	[0.0215]
Bartik Wage	1.008***	0.398***	0.194***	1.074***	0.273***	0.0608	0.893***	0.422***	0.272*
	[0.0430]	[0.0570]	[0.0658]	[0.0453]	[0.0709]	[0.0622]	[0.0486]	[0.0924]	[0.153]
Bartik Employment	0.0531**	0.545***	0.363***	0.0968***	0.599***	0.255***	0.0151	0.475***	0.491***
	[0.0266]	[0.0379]	[0.0362]	[0.0330]	[0.0404]	[0.0259]	[0.0288]	[0.0449]	[0.0744]
Population	0.127	-0.443*	-0.424	-0.152**	0.105	-0.498	0.256*	-0.353*	-0.524
	[0.131]	[0.254]	[0.326]	[0.0704]	[0.112]	[0.383]	[0.144]	[0.204]	[0.350]
Constant	0.521***	0.133***	0.0618***	0.463***	-0.106***	-0.110***	0.487***	0.454***	0.471***
	[0.00283]	[0.00513]	[0.00507]	[0.00344]	[0.00624]	[0.00410]	[0.00327]	[0.00596]	[0.00940]
Observations	2,166	2,166	2,166	2,166	2,166	2,166	2,166	2,166	2,166
R-squared	0.973	0.916	0.875	0.964	0.9	0.858	0.965	0.931	0.854

Commuting Zone FE	YES	YES	YES	YES	YES	YES	YES	YES	YES
Year FE	YES	YES	YES	YES	YES	YES	YES	YES	YES
Population Weighted	YES	YES	YES	YES	YES	YES	YES	YES	YES

All standard errors clustered at the commuting zone level.

*** p<0.01, ** p<0.05, * p<0.1

Table 15: Robustness on Matched Sample Reduced Form Regressions: No Population Control

	Change Total Log Wage	Change Total Log Employment	Change Total Population %	Change LS Log Wage	Change LS Log Employment	Change LS Population %	Change HS Log Wage	Change HS Log Employment	Change HS Population %
Predicted Refugees	-0.00853	-0.00322	-0.00603	-0.00169	0.000482	-0.014	-0.02	-0.031	-0.0245
	[0.0140]	[0.0201]	[0.0230]	[0.0144]	[0.0212]	[0.0122]	[0.0152]	[0.0496]	[0.0595]
Bartik Wage	1.014***	0.466***	0.196***	1.092***	0.131*	0.0686	0.872***	0.352***	0.18
	[0.0657]	[0.0806]	[0.0522]	[0.0719]	[0.0731]	[0.0659]	[0.0832]	[0.130]	[0.175]
Bartik Employment	-0.00767	0.548***	0.279***	0.0312	0.716***	0.223***	-0.015	0.479***	0.476***
	[0.0749]	[0.0863]	[0.0644]	[0.0878]	[0.0884]	[0.0436]	[0.0742]	[0.0735]	[0.138]
Shift Share	-0.0318	-0.238***	0.0452	-0.188***	0.0523	0.0927	0.105*	-0.225	0.0384
	[0.0650]	[0.0789]	[0.0697]	[0.0672]	[0.0953]	[0.101]	[0.0554]	[0.216]	[0.188]
Constant	0.516***	0.118***	0.0469***	0.461***	-0.0879***	-0.102***	0.487***	0.444***	0.452***
	[0.00371]	[0.00735]	[0.00533]	[0.00384]	[0.00491]	[0.00549]	[0.00421]	[0.0113]	[0.0140]
Observations	1,093	1,093	1,093	1,093	1,093	1,093	1,093	1,093	1,093
R-squared	0.97	0.901	0.872	0.957	0.888	0.883	0.96	0.908	0.835

Commuting Zone FE	YES	YES	YES	YES	YES	YES	YES	YES	YES
Year FE	YES	YES	YES	YES	YES	YES	YES	YES	YES
Population Weighted	YES	YES	YES	YES	YES	YES	YES	YES	YES

All standard errors clustered at the commuting zone level.

*** p<0.01, ** p<0.05, * p<0.1